RACIAL SEGREGATION AND THE BLACK-WHITE TEST SCORE GAP*

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ABSTRACT

Racial segregation is often blamed for part of the achievement gap between blacks and whites. In this paper we study the effects of school and neighborhood segregation on the relative SAT scores of black students across different metropolitan areas, using large microdata samples for the 1998-2001 test cohorts. Without controlling for neighborhood segregation, we find that school segregation is negatively associated with black relative test scores, and also with relative education and employment outcomes measured in the 2000 Census. In models that include both school and neighborhood segregation, however, the effect of relative exposure to black schoolmates is uniformly small and statistically insignificant, while neighborhood segregation has a strong negative effect. Instrumental variables estimates that isolate the components of school segregation associated with court-ordered desegregation plans or the geographic features of a city are consistent with this result but imprecise. Models that include school segregation, neighborhood segregation, and measures of the relative exposure of blacks to other characteristics of their neighbors (e.g. education and income) show weaker effects of neighborhood segregation, suggesting that the socio-economic status of neighbors, rather than their race, may be the primary source of these effects.

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I. INTRODUCTION

The racial gap in student achievement is a pervasive and divisive feature of American life. Black-white differences in standardized test scores lie at the core of the debate over affirmative action in college admissions (Bowen and Bok, 1998; Kane, 1998) and public sector hiring (McCrary, 2004).[4] The racial gap in test scores also figures prominently in the recent No Child Left Behind Act.[5] Many years before the Supreme Court's *Brown v. Board* decision, segregation was identified as a possible explanation for lower black achievement.[6] Consistent with this idea, studies since the Coleman Report (Coleman, 1965) have found that test scores are lower at schools with higher black enrollment shares.[7] Likewise, there is a strong negative correlation between education outcomes and the fraction of black residents in a neighborhood (e.g., Massey and Denton, 1993).

Establishing whether segregation actually *causes* lower black achievement is difficult, however, because individuals are not randomly assigned to neighborhoods or schools. A credible research design has to address the possibility that students who attend schools with a larger black enrollment share – or live in predominantly black neighborhoods – are different from those in other schools and neighborhoods, and that these differences contribute to their lower achievement.[8] One approach is to focus on city-wide averages. Assuming that average student ability (conditional on race) is the same in different cities, cross-city comparisons can identify the effects of school or neighborhood composition on

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[4] Hartigan and Wigdor (1987) present an overview of the use and implications of standardized testing in the labor market.

[5] Title I of the No Child Left Behind Act gives as one of its purposes "…closing the achievement gap between high- and low-performing children, especially the achievement gaps between minority and nonminority students." The full text of the Act is available at http://www.ed.gov/policy/elsec/leg/esea02/pg1.html

[6] Crowley (1932) presents an early study of the effect of racially segregated schools on academic achievement, based on comparisons of test scores for black students in an all-black and a mixed-race high school in Cincinnati. She finds no difference between the schools.

[7] See Ferguson (1998) and Hanushek, Kain and Rivkin (2002) for more recent studies.

[8] On the general problem of inferring peer group effects from observational data, see Manski (1993) and Brock and Durlauf (2001).

student outcomes. Evans, Oates, and Schwab (1992), for example, studied the effects of economically disadvantaged peer groups on teenage pregnancy and dropout behavior, using city-wide averages of income, education, and unemployment as instrumental variables for the fraction of disadvantaged peers at a student's school.[9]

An important limitation of the cross-city research design is that the demographic composition of a city may be correlated with unobserved characteristics that influence mean outcomes. In this paper we build on an idea of Cutler and Glaeser (1997) and relate the achievement *gap* between black and white students in a city to *differences* in their exposure to black peers in neighborhoods and schools.[10] Comparisons between the black and white students in a city eliminate the effects of any city-wide variables that may be correlated with racial segregation, such as the level of school funding or the administrative efficiency of local schools. We also control directly for observed differences in the family background characteristics of white and black students in different cities, and for many other characteristics, including region and racial composition, that may affect black relative performance across cities. We apply this approach to an unusually rich micro data set containing SAT scores for black and white high school students who wrote the test between 1998 and 2001.

Unlike previous studies we attempt to separately identify the effects of school and neighborhood segregation on black relative achievement, using data on school racial

[9] Other recent studies that use geographically aggregated outcomes include Hoxby (2000), who examines the effects of changes in the number of school districts on city-wide test scores, and Hsieh and Urquiola (2003), who use area-wide scores to examine the impact of private school competition.

[10] Cutler and Glaeser (1997) compare the educational attainment and labor market outcomes of blacks and whites living in cities with differing levels of residential segregation. We extend their approach by distinguishing between neighborhood and school segregation, by controlling for a much larger set of individual and city characteristics that may influence the within-city black-white gap; and by focusing on educational achievement rather than attainment or labor market success.

composition from the Common Core of Data and the Private School Survey[11] and data on Census tract composition from the 2000 Census. A key concern is that intercity differences in either dimension of segregation may be endogenously related to black-white differences in unobserved characteristics that also affect test scores. To address this concern we use an instrumental variables approach to isolate the part of current residential segregation that is explained by historical segregation, and the parts of current school segregation that are attributable to geographic features or to court-ordered desegregation programs adopted in the 1970s and early 1980s.[12] Finally, we test whether the estimated segregation effects reflect race *per se*, or whether racial segregation is only a proxy for differential exposure to other peer characteristics (e.g. family income) that are correlated with race.

Our empirical analysis leads to two main conclusions. First, although black relative test scores are negatively correlated with measures of school segregation, when controls are added for neighborhood segregation the effects of school segregation become uniformly small and statistically insignificant. Specifications that instrument either segregation measure confirm this pattern. Second, neighborhood segregation has robust negative impacts on black relative test scores, though these also fall in size and significance once controls are added for the relative exposure of black and white children to neighboring families with differing income, education, and marital status. Thus, as suggested by Wilson (1987), race may not be the primary source of neighborhood segregation effects: rather, racial segregation may proxy for relative exposure to economically successful neighbors.

While SAT scores play a critical role in regulating entry to higher education, a

[11] These are censuses of public and private schools, respectively, conducted by the National Center for Education Statistics. The Common Core of Data (CCD) is annual, while the Private School Survey (PSS) is less frequent. Each reports the size of each school, the enrollment in each grade, and the racial composition.

[12] Guryan (2004) shows that the implementation of a major desegregation program during the 1970s was associated in the short term with a modest but statistically significant 3 percentage point reduction in black dropout rates relative to whites, suggesting that policy-induced changes in school-level racial segregation had an impact on black students' relative performance.

drawback of the SAT is selective test participation. We eliminate the most extreme selection biases by excluding data from states where a majority of college-bound students write the ACT test (Clark, 2003). We also re-weight the observed test scores to adjust for differences in SAT participation rates across schools, and include a selection correction based on average school-level participation rates. As a check on the robustness of our findings, we estimate a series of models using 2000 Census data on employment and schooling outcomes of 16-24 year olds. Estimates from these models are strikingly parallel to our SAT results, suggesting that selective test participation is not driving our main conclusions.

The finding that school segregation has no effect on black relative achievement once controls are added for neighborhood segregation leads us to consider three potential mechanisms that might confound the true effect of school desegregation: unobserved differences in school quality, unobserved differences in schoolmate characteristics, and *within-school* segregation.[13] Using data from the CCD and the Schools and Staffing Survey, we conclude that there is no strong relationship between school segregation and observable indicators of the relative quality of the schools attended by black students. To evaluate the potential influence of unobserved schoolmate characteristics we use school-level data on the fraction of students who participate in free or reduced price lunch programs. We find that school segregation is highly correlated with black-white relative exposure to low-income schoolmates. To the extent that poor schoolmates lower achievement, however, this pattern would tend to *reinforce* any causal effect of school segregation, and thus cannot explain our finding of no effect.

Finally, we study within-school segregation patterns using data on the relative

[13] Cohen (2004) provides a recent account of within-school segregation issues at Evanston Township High School. A 1993 suit filed in Rockford Illinois led to a court order to integrate the **courses** offered at middle and high schools in the district (Weiler, 2004). See also the discussions in Clotfelter, Ladd, and Vigdor (2003) and Clotfelter (2004).

participation of black and white students in honors and advanced placement (AP) classes. Holding constant the level of neighborhood segregation, we find that the black-white gap in honors and AP participation is wider in cities with more racially integrated schools. This pattern is consistent with claims that ability tracking and related programs offset the integrative effects of between-school desegregation efforts, and may help to explain why differences in school segregation do not appear to influence black relative achievement.

II. EMPIRICAL FRAMEWORK

A. Basic Model

As a starting point for analyzing the effects of racial segregation on the black-white achievement gap, we begin with a model that expresses the test score of a given student as a function of his or her own characteristics, the characteristics of his or her schoolmates and school, the characteristics of his or her neighborhood, the racial composition of the school and neighborhood, and an unobserved error with a school-level component that may vary by race. Specifically, we posit:

$$(1) \qquad y_{ijsc} = X_{ijsc}\alpha_j + Z_{sc}\beta_j + W_{ijsc}\nu_j + B_{sc}\gamma_j + R_{ijsc}\delta_j + u_{jsc} + \varepsilon_{ijsc},$$

where y_{ijsc} represents the test score (or some alternative measure of achievement) of student i of race group j in school s and city c, X_{ijsc} is a vector of observed characteristics of the student, Z_{sc} is a vector representing the average characteristics of the students in school s and other features of the school, W_{ijsc} is a vector of the average characteristics of i's neighbors, B_{sc} represents the fraction of black students in school s, R_{ijsc} is the fraction of black residents in student i's neighborhood, u_{jsc} is a shared error component for students of group j in school s and city c, and ε_{ijsc} is an individual-level error (with mean 0 for each race group in each school). Racial segregation effects arise in model (1) through the γ_j and δ_j

coefficients, which measure the effects of exposure to black schoolmates or black neighbors on student achievement *holding constant the other characteristics of schoolmates and neighbors*.[14] As we show momentarily, equation (1) implies that the test score *gap* between blacks and whites in a city depends on the degree of racial segregation of schools and neighborhoods, and on other contextual factors, such as the gap in average incomes between the neighbors of a typical black student and a typical white student.

In principle equation (1) can be estimated by ordinary least squares (OLS) using student-level data. The problem with such a strategy (noted by Evans, Oates, and Schwab, 1992, and Cutler and Glaeser, 1997) is that any non-randomness in the sorting of students to schools or neighborhoods is likely to bias the resulting estimates of γ_j and δ_j. In particular, if students with higher unobserved abilities attend schools with a lower fraction of black students, the combined error component ($u_{jsc} + \varepsilon_{ijsc}$) will be negatively correlated with B_{sc}, leading to a negative bias in the estimate of γ_j. Likewise, selective residential sorting is likely to produce a negative bias in the estimate of δ_j.

To eliminate the biases arising from within-city sorting, we aggregate achievement outcomes by race group to the city level and then compute the city-level difference between blacks and whites. Specifically, equation (1) implies that the mean outcome of race group j in city c is:

$$(1') \qquad y_{jc} = X_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\nu_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc},$$

where X_{jc} represents the mean characteristics of students of group j in city c, Z_{jc} and W_{jc}

[14] A simple peer group model could lead to such effects. For example, if student test scores are affected by the average scores of their peers, a rise in the fraction of black schoolmates or neighbors will tend to lower test scores, since black students have lower average test scores than whites. Alternatively, if black students value academic achievement less than whites (Ogbu and Forham, 1986; Ogbu, 2003), and if individual student performance is affected by peer norms, a rise in the fraction of black classmates or black neighbors will tend to lower achievement. Austen-Smith and Fryer (2003) present a model in which black children reduce their effort to avoid peer group rejection. Their model predicts a nonlinear effect of racial composition on black outcomes.

represent the mean characteristics of the school-level and neighborhood-level peer groups of race-j students, B_{jc} is the average fraction of black students at schools attended by race group j in city c, R_{jc} is the average fraction of black neighbors of students in group j in city c, and u_{jc} is the mean "unobserved ability" of students of race j in city c. The difference in mean outcomes between black and white students in city c is thus given by:

$$(2)\qquad y_{1c} - y_{2c} = X_{1c}\alpha_1 - X_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\nu_1 - W_{2c}\nu_2 + B_{1c}\gamma_1 - B_{2c}\gamma_2$$
$$+ R_{1c}\delta_1 - R_{2c}\delta_2 + u_{1c} - u_{2c},$$

where j=1 represents blacks and j=2 represents whites.

If all the coefficients in equation (1) are the same for whites and blacks then equation (2) takes a particularly simple form:

$$(2')\qquad \Delta y_c = \Delta X_c\alpha + \Delta Z_c\beta + \Delta W_c\nu + \Delta B_c\gamma + \Delta R_c\delta + \Delta u_c ,$$

where Δy_c, for example, denotes the difference in mean test scores between blacks and whites in the same city. The differences ΔB_c and ΔR_c are closely related to standard measures of the racial segregation of schools and neighborhoods in a city.[15] In particular, full racial segregation implies that $B_{1c} = R_{1c} = 1$ and $B_{2c} = R_{2c} = 0$, leading to values for ΔB_c and ΔR_c of 1. At the other extreme, complete racial integration implies that $B_{1c} = B_{2c}$, $R_{1c} = R_{2c}$, and $\Delta B_c = \Delta R_c = 0$. The differences ΔZ_c and ΔW_c measure other potentially important differences in the characteristics of the schoolmates and neighbors of black and white children. For example, if W includes the average family income in a neighborhood, then ΔW_c includes the difference between the average incomes in the neighborhoods of black and white children.

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[15] In the segregation literature (e.g. Massey and Denton 1988; Iceland, Weinberg et al. 2002), B_{jc} and R_{jc} are known as indices of exposure of race-j students to blacks, and ΔB_c and ΔR_c (or versions of them that scale by the city fraction black—see Cutler, Glaeser and Vigdor 1999) are sometimes known as isolation indices. We do not rescale by the city fraction black, but as explained below we control separately for B_c, the overall fraction of black students in a city.

The strategy of aggregating and differencing between races eliminates any bias caused by the endogenous sorting of students to schools within a given city, and also eliminates any city-wide variables that affect the two race groups equally. Nevertheless, it is still possible that there are unobserved differences between the black and white students in a city. We posit that the remaining unobserved ability gap can be decomposed as:

(3) $u_{1c} - u_{2c} = F_c\psi + v_c$,

where F_c is a vector of city characteristics (including region dummies, the mean and dispersion in family income in a city, and the overall fraction of black students in the city) and v_c represents all remaining unobserved differences between black and white students in city c. Assuming that the differenced specification (2') is valid, this leads to a model of the form:

(4) $\Delta y_c = \Delta X_c \alpha + \Delta Z_c \beta + \Delta W_c \nu + \Delta B_c \gamma + \Delta R_c \delta + F_c \psi + v_c$.

OLS estimation of this equation will yield consistent estimates of γ and δ if (and only if) v_c, the unexplained difference in black-white test outcomes, is uncorrelated with ΔB_c and ΔR_c conditional on the other control variables included in (4).

Consideration of equation (4) suggests a number of possible threats to the identification of the racial segregation effects γ and δ. One is the non-random sorting of black and white families to different metropolitan areas. If achievement-oriented black families tend to migrate to cities where schools or neighborhoods are less racially segregated, and if their characteristics are not fully captured in the measured student background variables, then v_c may be negatively correlated with ΔB_c and/or ΔR_c. As a partial control for this problem, we include in F_c an estimate of the difference in the mean residual wage gap between black and white parents in city c. To the extent that the mean wage residual is a measure of the "unobserved ability" of the parents of a given race group in a given city, and that the unobserved determinants of children's academic achievement are correlated with their parents' unobserved ability, the mean residual wage gap will control for non-random sorting of black and white families to different cities.[16]

Another potential source of endogeneity bias is that the degree of segregation in a city – particularly in a city's schools -- may depend on the black-white gap in unobserved ability. To assess this possibility we use instrumental variables for school and neighborhood segregation. Specifically, we examine two instruments for school segregation: the number of rivers and streams in an MSA (which is strongly correlated with the number of school districts in the MSA, and due to the role of district boundaries in desegregation efforts, also correlated with the degree of across-school racial segregation),[17] and an estimate of the "bite" of Court-ordered school desegregation programs in the city in the 1970s and early 1980s, computed from data reported by Welch and Light (1987). We also estimate models that instrument the degree of current neighborhood segregation with a measure of segregation in 1980.

A third specification issue is the presence of unobserved differences in the quality of the schools attended by black and white students. Any systematic correlation between the degree of racial segregation of a city's schools and the quality differential between schools attended by black and white students will lead to a bias in the estimate of γ. Specifically, if school quality has a positive effect on achievement, and if black schools are of lower relative quality in more segregated cities, specifications that ignore school quality will lead to negatively biased estimates of the school segregation effect. The omission of school quality

[16] We have also used data on migration flows for blacks and whites in the National Longitudinal Survey of Youth (NLSY) to examine the impacts of racial segregation on relative migration patterns of blacks and whites with differing AFQT scores. These data show no indication of selective migration flows.

[17] Cutler and Glaeser (1997) use streams as an instrument for residential segregation. We find no significant first stage relationship between residential segregation and streams in our samples. In any case, the existence of a relationship between streams and residential segregation would not be a problem for us, since we control for residential segregation in our IV analysis of the effect of school segregation.

may also lead to some bias in the neighborhood segregation effect. In specifications that include both segregation measures, however, we would expect most of the bias to be concentrated on the school segregation effect. In section V, below, we confirm this conjecture by examining data on observed indicators of school quality.

A similar argument applies to the effect of omitted neighborhood characteristics. It seems plausible—and we confirm empirically below—that the gap in measures of neighborhood quality between blacks and whites in a city is highly correlated with the degree of racial segregation of its neighborhoods. Specifications that omit key neighborhood characteristics, then, will lead to measured neighborhood segregation effects that incorporate both the direct effect of exposure to black neighborhoods as well as the effect of any gap in the quality of neighborhoods where black and white families reside.

B. Controlling for Student-Level Covariates

Family background variables like parental education have a strong influence on student achievement and it is important to control for differences in the family background characteristics of black and white students in different cities as flexibly as possible. Since the aggregated model (4) has only as many degrees of freedom as the number of metropolitan areas in the sample, a highly flexible specification is not feasible. Instead, we take advantage of the individual-level test score data to partial out the student-level covariates observed in the SAT files (mother's education, father's education, and family income) before aggregating to the city level. We estimate separate student-level models for white and black test takers that include unrestricted school effects and a highly flexible specification for these covariates:

$$Y_{ijsc} = \zeta_{jsc} + f_j(X_{ijsc}) + \varepsilon_{ijsc}.$$

We then form an adjusted test score for each student:

$$r_{ijsc} = Y_{ijsc} - \hat{f}_j(X_{ijsc}),$$

and consider a city-level model for the difference in mean adjusted test scores:

$$r_{1c} - r_{2c} = \Delta X'_c \alpha + \Delta Z_c \beta + \Delta W_c \nu + \Delta B_c \gamma + \Delta R_c \delta + F_c \psi + v_c + e_{1c} - e_{2c}, \tag{5}$$

where $e_{jc} = f_{jc} - \hat{f}_{jc}$, f_{jc} represents the mean of $f_j(X_{ijsc})$ for students of race j in city c, $\hat{f}_{jc}$ represents its estimated counterpart; and $\Delta X'_c$ includes black-white differences in a limited selection of background variables (including $\hat{f}$, linear measures from the SAT data, and several background variables that are not observed in the SAT files but can be constructed from census data). Although the first stage adjustment may not fully eliminate the effect of the X^1 variables, we anticipate that the inclusion of $\Delta X'_c$ in the second stage model absorbs most of the remaining variation in Δe_c.

C. Adjusting For Selective Participation in the SAT

SAT participation rates vary across cities, leading to potential selectivity biases in the observed mean scores. These biases may be particularly large in states where most college-bound students write the ACT rather than the SAT. In our empirical analysis we therefore focus exclusively on data from "SAT states." Even in these states, citywide SAT participation rates range from 20 to 60 percent. With selective participation, the average scores for all test-takers in a city will tend to under-represent the outcomes for students at "low performing" schools, where both scores and participation are lower, with greater under-representation in cities with lower overall participation.[18] As pointed out by Gronau

[18] The correlation of SAT-taking rates and average scores across schools is positive in our data, the opposite sign of what would be implied by a conventional selection model with positive selection into test-taking. We suspect that at the individual level the selection is positive, but that there are large differences in the unobserved determinants of participation rates and mean scores that dominate the correlation across schools.

(1974) and Heckman (1978), such a tendency may lead to biases in the measured effects of variables that influence scores, like school or neighborhood segregation. We attempt to reduce such biases by re-weighting the average scores from different high schools in a city to reflect their relative enrollments, and by adding a control function based on the mean SAT participation rate across high schools in a city.

These adjustments are motivated by an underlying model of test participation in which the unobserved component of individual test participation is potentially correlated with the unobserved component of test scores. Specifically, we assume that the probability that student i in race group j in school s in city c writes the SAT is given by a latent index model of the form:

$$(6) \qquad P(\text{i writes test} \mid X_{ijsc};\ s,j,c) = p_{ijsc} = P(X_{ijsc}\,\pi_j + \mu_{ijsc} \geq k_{jsc}),$$

where μ_{ijsc} is an error component and k_{jsc} is a school and group-specific threshold. Assuming that μ_{ijsc} and the error in the test score model (ε_{ijsc}) are jointly normally distributed, with a distribution that is constant across schools (but may vary by race) the expected test score for student i in group j in school s, conditional on writing the test, is

$$(7) \qquad E[\,y_{ijsc} \mid \text{i writes test}, X_{ijsc};\ s,j,c\,] = X_{ijsc}\alpha_j + Z_{sc}\beta_j + W_{ijsc}\nu_j + B_{sc}\gamma_j + R_{ijsc}\delta_j + u_{jsc} + \zeta_j\,\lambda(p_{ijsc}),$$

where $\lambda(p)$ is the inverse Mills ratio function evaluated at $\Phi^{-1}(p)$ and ζ_j is a race-specific coefficient that depends on the correlation of μ_{ijsc} and ε_{ijsc}. The adjusted observed test score for individual i is therefore:

$$(8) \qquad r_{ijsc} = X'_{ijsc}\alpha_j + Z_{sc}\beta_j + W_{ijsc}\nu_j + B_{sc}\gamma_j + R_{ijsc}\delta_j + u_{jsc} + \zeta_j\,\lambda(p_{ijsc}) + e_{ijsc},$$

where e_{ijsc} combines the estimation error in $\hat{f}_j$ and the deviation of y_{ijsc} from its conditional expectation.

A simple average of the observed test scores in a city will contain a participation-weighted average of the u_{jsc}'s that may differ from the unconditional mean u_{jc}. We therefore consider an enrollment-weighted average of the observed residual test scores:

$$r_{jc} = 1/N_{jc}\ \Sigma_s\ N_{isc}\, r_{jsc} = 1/N_{jc}\ \Sigma_s\, N_{jsc}/M_{jsc}\, \Sigma_i\, r_{ijsc} = 1/N_{jc}\ \Sigma_s\, \Sigma_i\, p_{jsc}^{-1}\ r_{ijsc}\ ,$$

where N_{jc} is the total number of 12[th] graders of group j in city c, N_{jsc} is the number of 12[th] graders in school s, M_{jsc} is the number of test-takers in group j in school s, and $p_{jsc} = M_{jsc}/N_{jsc}$ is the test participation rate of group j in school s. Equation (8) implies that:

$$\begin{aligned}(9)\qquad r_{jc} = {} & X'_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\nu_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc} + \\ & \zeta_j\,(1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\,\lambda(p_{ijsc}) + e_{jc}\,,\end{aligned}$$

where Z_{jc}, W_{jc}, R_{jc}, B_{jc} and u_{jc} are the same as in equation (2).

Next, consider a first order expansion of the selection-correction function for individual i around p_{jsc}, the test participation rate for students of group j in school s:

$$\lambda(p_{ijsc}) = \lambda(p_{jsc}) + (p_{ijsc} - p_{jsc})\,\lambda'(p_{jsc}) + \xi_{ijsc}\,.$$

For a range of probabilities between 0.2 and 0.8 the function $\lambda(p)$ is approximately linear and the error ξ_{ijsc} is small.[19] Using this expansion:

$$\begin{aligned}(1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\,\lambda(p_{ijsc}) &= (1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\ \{\lambda(p_{jsc}) + (p_{ijsc} - p_{jsc})\,\lambda'(p_{jsc}) + \xi_{ijsc}\} \\ &= \lambda_{jc} + \theta_{jc} + \xi_{jc}\,,\end{aligned}$$

where

$$\lambda_{jc} = (1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\ \lambda(p_{jsc})\,,$$

$$\xi_{jc} = (1/N_{jc})\,\Sigma_s\,\Sigma_i\, p_{jsc}^{-1}\ \xi_{ijsc}\ ,$$

$$\begin{aligned}\theta_{jc} &= (1/N_{jc})\,\Sigma_s\ N_{jsc}\ \lambda'(p_{jsc})\ (1/N_{jsc})\,\Sigma_i\ (p_{ijsc} - p_{jsc}) \\ &= (1/N_{jc})\,\Sigma_s\ N_{jsc}\ \lambda'(p_{jsc})\ \{p^T_{jsc} - p_{jsc}\},\end{aligned}$$

and p^T_{jsc} is the average test participation probability *among the test writers of group j in school s.*

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[19] In the range between 0.2 and 0.8, $\lambda(p)=1.76-1.81p$. Nearly two thirds of schools in our sample have participation rates in this range.

Note that the first term, λ_{jc}, is just an enrollment-weighted average of the inverse Mills ratio functions evaluated at the (race-specific) test participation rates at each school. The second term, ξ_{jc}, is an average approximation error, which we expect to be small. The third term, θ_{jc}, is more problematic. This term measures the degree of "within-school" selectivity of test-takers, and will be strictly positive unless test participation is random within a school.

Combining these results with equation (9), an approximate expression for the average adjusted test score for group j in city c is:

$$r_{jc} = X'_{jc}\alpha_j + Z_{jc}\beta_j + W_{jc}\nu_j + B_{jc}\gamma_j + R_{jc}\delta_j + u_{jc} + \zeta_j\lambda_{jc} + \zeta_j\theta_{jc} + e_{jc} .$$

Differencing between blacks and whites in the same city and substituting equation (3) for the difference in the unobserved ability components leads to:

$$(10) \quad \Delta r_c = r_{1c} - r_{2c} = X'_{1c}\alpha_1 - X'_{2c}\alpha_2 + Z_{1c}\beta_1 - Z_{2c}\beta_2 + W_{1c}\nu_1 - W_{2c}\nu_2$$
$$+ B_{1c}\gamma_1 - B_{2c}\gamma_2 + R_{1c}\delta_1 - R_{2c}\delta_2 + F_c\psi + \zeta_1\lambda_{1c} - \zeta_2\lambda_{2c}$$
$$+ \zeta_1\theta_{1c} - \zeta_2\theta_{2c} + v_c + e_{1c} - e_{2c} .$$

or, if the coefficients β, δ, γ, and ζ are the same for whites and blacks:

$$(10') \quad \Delta r_c = \Delta X'_c\alpha + \Delta Z_c\beta + \Delta W_c\nu + \Delta B_c\gamma + \Delta R_c\delta + F_c\psi + \zeta\Delta\lambda_c$$
$$+ \zeta\Delta\theta_c + v_c + \Delta e_c .$$

Equation (10') differs from the model that would prevail in the absence of selective test participation in two ways. First, it includes the differenced control function $\Delta\lambda_c$. Second, it includes as part of the residual the difference in the within-school selectivity terms for blacks and whites, $\Delta\theta_c$. If a rise in school or neighborhood segregation causes black relative test scores to fall but also causes a rise in the *relative* within-school selectivity of black test takers, the presence of this term will lead to attenuation in the estimated negative effect of segregation on relative test scores.

A final problem caused by selective test participation is that we cannot use the SAT data to estimate the average characteristics of the students at each school. Thus, we cannot in general estimate Z_{1c} and Z_{2c}. To the extent that the relevant peer group for the SAT-takers in a school is the set of SAT-takers of the same race in that school, Z_{jc} will be proxied by X^1_{jc}, and we can reinterpret the measured effect of the observed student-level characteristics as representing a combination of "own" and peer group effects (i.e., as representing $\alpha + \beta$). If peer groups include non-test-takers, or stretch across race groups within a school, however, ΔX^1_c will not fully absorb the differences in schoolmate characteristics of black and white test takers, and unobserved differences in school peers may load onto other variables in the model. In section III, below, we examine one available measure of school-wide student characteristics – the fraction receiving free or reduced price lunches – and find that relative exposure to schoolmates receiving subsidized lunches is highly correlated with relative exposure to black schoolmates. Assuming that low-income peers depress performance, omission of this measure from our primary equation will thus lead to a negatively biased estimate of the direct effect of segregation on black relative outcomes.

III. DATA SOURCES AND SAMPLE OVERVIEW

A. Data Sources

Our primary source of student achievement data is a sample of SAT records for 25% of white test takers and 100% of black test-takers in the 1998-2001 SAT test cohorts.[20] These files include test scores as well as self-reported demographic and family background information. They also include high school identifiers, which we use to match school-level

[20] We also have and use observations on 100% of white test takers in California and Texas. Though the re-weighting discussion above omits sampling weights, we use them in all computations of test score or student characteristic averages. We exclude observations for students who reported ethnicity other than white or black (primarily Hispanics and Asians) and those who did not report their race/ethnicity.

information from the appropriate editions of the Common Core of Data (CCD, for public school students) and the 1997-8 Private School Survey (PSS). To minimize the impact of measurement errors in enrollment counts, which appear to be quite variable over time in the CCD, we estimate the number of students, the number of test takers, and the racial composition of each school using averages over the four years in our data. We assign students to Metropolitan Statistical Areas (MSAs) based on year-2000 definitions, using location information in the CCD and PSS files.[21] As noted earlier, we restrict our analysis of SAT outcomes to MSAs in states with overall test participation rates of 25% or higher, which we refer to as "SAT states."[22]

Our primary dependent variable is the gap in SAT scores between black and white students in the same MSA, adjusted for three student background factors reported by SAT takers – mother's education, father's education, and income.[23] We form an enrollment-weighted average of the residual scores for black and white students in each city and then take the difference between the mean adjusted scores for blacks and whites as our primary dependent variable. We also estimate some models for the black-white gap in estimated test participation rates.

We construct estimates of the average fraction of black schoolmates for black and

[21] Where the Office of Management and Budget designates a larger metropolitan area as a Consolidated Metropolitan Statistical Area (CMSA) with several sub-areas (Primary Metropolitan Statistical Areas, or PMSAs), we treat the PMSA as the relevant city definition. In every specification, however, we estimate standard errors that are "clustered" by CMSA.

[22] The cutoff for defining "SAT states" was developed by examining the distribution of state-level SAT participation rates, which is bi-modal; states with lower SAT participation rates uniformly have high participation rates for the ACT exam, a competing college entrance test that is widely used in the Midwestern states.

[23] The adjustment procedure uses race-specific regression models that include unrestricted high school dummies and 114 parental background dummies, formed from the 14 income categories reported in the SAT and the full interaction of the 10 categories for each parent's education. The income and education categories include "missing" as one possibility. Our adjusted scores are composed of the residuals from these regressions plus the estimated school effects.

white high school students in each MSA using school-level data from the CCD and PSS.[24] We also compute a similar measure of the relative exposure of black and white students to Hispanic schoolmates.[25] Finally, we construct parallel measures of neighborhood-level exposure to black and Hispanic neighbors using Census tracts as the unit of exposure and drawing tract-level population counts by race and ethnicity from the 2000 Census full population counts (Summary File 1).[26]

For some supplementary analyses, we use the 2000 Census PUMS to construct alternative measures of black-white gaps in academic outcomes (such as high school completion) that are not subject to biases that selectivity into SAT-taking may introduce. We also use data from Cutler, Glaeser, and Vigdor (1999) on historical residential segregation patterns; from Welch and Light (1987) on major desegregration programs; and from the CCD and Schools and Staffing Survey (SASS) on teacher characteristics at schools attended by black and white students.

Finally, we use the 2000 Census to estimate some family background characteristics for black and white students in each city. Where possible, we use the Summary Files to get data for white and black families by MSA (based on the full 1-in-6 set of respondents who fill out the Census long form). To measure parental education, however, we construct our own estimates from the 5-percent public use samples by merging mother's and father's data

[24] When we analyze outcomes that are only available for public schools or for which we cannot readily distinguish different grades (e.g. teacher-student ratios), we use school segregation measures computed over the relevant schools and grade levels.

[25] Several of our data sets treat Hispanic as a distinct racial category. In other data sets, we do the same, excluding Hispanics from both the white and black groups. In 2000 Census data, where possible we include multi-race non-Hispanics as blacks if they report black as one of their races; we never count multi-race individuals as white.

[26] Census tracts are initially defined to encompass demographically homogenous neighborhoods of about 4,000 residents. Once tract boundaries are drawn, however, the Census Bureau attempts to keep them constant over time. As a check on the use of tracts to define "neighborhoods", we also construct exposure measures based on Census Block Groups (which have typical populations of about 1000 residents). Relative exposure indexes constructed from block groups and tracts are nearly perfectly correlated across cities and lead to virtually identical estimates.

to their children's records, and summarizing the results by race of the child and MSA. Further details on our data sources and merging methods are presented in a Data Appendix, available on request.

B. Overview of Sample

Table 1 gives an overview of the patterns of segregation and test scores for a selection of cities with low and high levels of residential segregation. The first three columns of the table show the absolute and relative levels of exposure of black and white residents in each city to black neighbors, while the next three columns show parallel measures of within-school exposure to black schoolmates. Finally, the three right-hand columns show average SAT scores for black and white test-takers in the city, and the racial gap in scores. Test scores are only reported for MSA's in "SAT states."

The upper panel of the table presents data for the five cities with the lowest levels of residential segregation. These are all smaller cities with very low black population shares. High schools in these cities tend to be a little more segregated than neighborhoods, though the black-weighted and white-weighted fractions of black students are uniformly small. The second panel lists the five least residentially segregated cities that have black population shares of at least 10%. These cities are all in the South. Even in these relatively integrated cities blacks are unevenly distributed across Census tracts, with at least an eight percentage point gap between the fraction of blacks in the tract of a typical black resident and that of a typical white resident. The black-white gap in relative exposure to black schoolmates (column F) tends to be very similar to the gap in relative exposure to black neighbors (column C).

Finally, the bottom panel of Table 1 lists the ten most residentially segregated metropolitan areas in the nation. This group includes cities like Gary, Indiana and Detroit, Michigan, in which black residents and black students are concentrated in a few neighborhoods and schools, leading to a very wide gap in the relative exposure of blacks and whites to black schoolmates.[27] It also includes some cities like St. Louis and Milwaukee where schools are notably less segregated than are neighborhoods. Though only two of the ten most segregated cities are in "SAT states" the average black-white test score gap in these cities is larger than the mean gap for the four cities in SAT states in the middle panel.

Table 2 presents some comparisons between the students in all 331 MSAs (columns A-B), those in the 189 cities from SAT states that are included in our analysis sample (columns C-D), and those in the 142 cities that are excluded from our test score samples (columns E-F). Blacks are slightly under-represented in the SAT state cities (11% of the student population versus 12% overall) whereas Hispanics are over-represented (25% of students versus 21% overall).[28] Cities from SAT states also have slightly lower rates of racial segregation at both the neighborhood and high school levels. 43 percent of white high school students and 31 percent of black high school students from cities in the SAT states write the SAT.

The bottom two rows in Table 2 show average SAT scores for the different city groups and the mean test gap between whites and blacks. Average SAT scores are lower in high-participation states (Dynarski 1987; Rothstein 2004), but the black-white difference is very similar for cities in SAT and non-SAT states, suggesting that use of within-city

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[27] It is worth noting that Gary is one of the few large cities with a sizable black population share where there was never a successful lawsuit or consent decree mandating active desegregation of the schools (Welch and Light 1987). Recall also that we always treat "cities" as MSAs; while Detroit and Gary have very few whites within their municipal boundaries, each MSA is about two thirds white.

[28] California, Texas, and Florida – the three states with the highest fractions of Hispanics – are all SAT states. In Table 2 (and in the remainder of our analysis), cities are weighted by $(1/N_{bc} + 1/N_{wc})^{-1}$ where N_{bc} and N_{wc} are the numbers of blacks and whites in the city population. These weights are inversely proportional to the variance of city-level differences between black and white averages in any representative sample from each city's population. Cities with very few blacks, such as those in Panel 1 of Table 1, receive very low weights.

differences may moderate problems associated with selective test participation.

As a final descriptive exercise, Figures 1 and 2 show the correlations across cities between the black-white adjusted test score gap and the relative segregation of schools (Figure 1) and neighborhoods (Figure 2) in a city. The scatter of points in each graph suggests a negative relationship between relative segregation and relative test scores of black students. [29]

IV. Regression Models for Black-White Gaps in Participation and Scores

A. Basic Models

We turn to the task of estimating equation (10') using city-level data for MSAs in high SAT participation states. Table 3 presents an initial set of estimates that exclude neighborhood-level segregation effects. All the models include the relative exposure of blacks and whites to black schoolmates and a parallel term representing relative exposure to Hispanic schoolmates, as well as main effects for the overall fraction black and Hispanic in the city's schools. All models—here and throughout the paper—also include 11 basic city-level control variables (log of city population, log of city land area, the fractions of city residents with 13-15 and 16+ years of education, log mean household income, the Gini coefficient of household income in the city, and dummies for 5 Census divisions[30]).

The models in columns A and B explore the effects of school segregation on SAT participation rates. Column A presents a baseline specification that includes only the basic city-level controls, with no additional student background characteristics. This specification

[29] The MSA with the most segregated schools is Gary, Indiana. Newark, New Jersey is second. Graphs using the black-white gaps in *unadjusted* scores look very similar to Figures 1a and 1b.

[30] Although there are nine Census divisions, only six are represented among SAT states. In Table 3 and the remainder of the paper, we restrict the sample to cities (185 of the 189 cities in SAT states) for which we can construct black-white differences in family background characteristics, introduced in Column B, using the 2000 Census microdata sample.

shows a significant negative effect of relative exposure to black schoolmates on the black-white gap in test participation (row 1). The effect of relative exposure to Hispanic schoolmates appears to be of similar magnitude, but is less precisely estimated and insignificant (row 2). The effect of the overall fraction of black students in a city (row 3) is small and insignificantly different from zero, indicating that black-white participation gaps do not vary substantially with the average racial composition of schools, once *relative* exposure to black schoolmates is held constant. The fraction Hispanic main effect (row 4) does appear to be significant, however, indicating that blacks are relatively more likely to write the SAT in cities with more Hispanics.

The model in column B adds a set of additional controls for the black-white gaps in several family background variables, computed from 2000 Census data.[31] These background variables are jointly significant, and their addition substantially reduces the size of the estimated school segregation effects. Evidently, most of the apparent correlation between school segregation and relative SAT participation is attributable to differences in the relative family background characteristics of black and white students in different cities. Once these are taken into account, there is little evidence that relative participation depends on relative school segregation, suggesting that selection biases in the relation between the test score gap and school segregation may be relatively minor.

Columns C-G present models for the gaps in adjusted test scores. Column C repeats the specification from Column A but adds a control for the black-white gap in the mean inverse Mills ratio (averaged across schools in the city) to absorb the between-school component of selection bias. The estimated selection coefficient is negative and significant, consistent with the hypothesis of positive selection into test-taking. The coefficient on

[31] Note that for the analysis of SAT participation it does not make sense to control for the relative characteristics of black and white SAT takers, since the population at risk includes all students in a city.

relative exposure to black schoolmates is also negative and is reasonably precisely estimated: Consistent with the simple scatterplot in Figure 1a, higher relative exposure of black students to black schoolmates (i.e. more segregation) is associated with lower black relative scores. The effect of relative exposure to Hispanic students is somewhat smaller, though still significant (and not significantly different from the black exposure effect), while the "main effects" of the overall fractions of black and Hispanic students in a city are small and insignificantly different from zero.

Columns D, E, and F add additional controls for black-white gaps in observable background characteristics, estimated from the SAT samples and 2000 Census data. The model in column D adds $\Delta\hat{f}_c$, our simple one-dimensional summary of the relative parental education and income differences between black and white test takers in different cities. This variable has a significant positive effect, and leads to a 20% reduction in the size of the estimated black exposure effect. Its addition also reduces the size of the inverse Mills ratio coefficient. The coefficient on the differenced background index is surprisingly large in magnitude, considering that that the dependent variable is already adjusted to remove the effects of individual test-takers' background characteristics. The index is measured in SAT points, so the 1.35 coefficient in column D implies that a 10 point widening in family background characteristics between the black and white high school students in a city increases the gap in *adjusted* test scores between black and white test takers by 13.5 points, and the gap in *actual* test scores by 23.5 points. Taken literally, this indicates that peer characteristics have a larger effect on a student's test scores than does his or her own family background. More plausibly, the coefficients used to form $\hat{f}$ may be attenuated, with the city average capturing some of the individual-level variation.

Column E shows that augmenting the model with estimates of the black-white differences in family background characteristics from the Census reduces the size of the estimated school segregation effects by nearly 50 percent (and also reduces the size of the coefficient of the relative family background index). Interestingly, once the Census controls are added the effect of our selection correction term switches sign, though it is not significantly different from zero. Finally, in column F we add some additional controls for the differences in the relative background characteristics of SAT takers, loosening the restriction implicit in the use of the estimated background index that these variables have the same relative effects across MSAs as they have within schools. Not surprisingly, this addition substantially reduces the precision of the background index's coefficient. It also reduces the magnitude of the estimated school segregation effects, so that relative exposure to black and Hispanic schoolmates is no longer statistically significant. We suspect that the model in column F is over-fit, since it includes 31 highly collinear explanatory variables in a model with only 185 observations. Moreover, the effects of some of the background variables are large and seemingly "wrong signed." For example, the large negative effects of the gaps in the fraction of fathers with a BA and mothers with some college measured from the SAT data seem implausible.

B. Estimating the Separate Effects of School and Residential Segregation

The results in Table 3 show that without controlling for neighborhood segregation, the effect of relative exposure to black schoolmates on the black-white test score gap is negative, though the precise magnitude of the effect varies across specifications. The next step is to augment these models with measures of neighborhood segregation. Table 4 presents a series of models parallel to those in Table 3, but including measures of school and neighborhood segregation. Column A presents a model for relative SAT participation that includes the same controls as the model in column B of Table 3, along with two additional variables representing the relative exposure of black residents to black and Hispanic neighbors. (For simplicity we do not report the effects of the student background variables) The neighborhood segregation effects are both negative, while their addition causes the estimated effects of exposure to black or Hispanic schoolmates to become positive. Interestingly, the Hispanic exposure effects are much larger than the black effects.

Column B extends the model in column A by including two additional city-level control variables: the black-white gaps in the mean wage residuals of mothers and fathers in the city.[32] The residual wage of fathers has a positive effect on SAT participation (consistent with the hypothesis that residual wages reflect unobserved ability, and that children of higher ability parents are more likely to write the SAT), though the effect of mothers' residual wages is essentially zero. More importantly, the addition of these extra controls has no noticeable effect on the size or precision of the estimated segregation effects.

Column C presents a final model for SAT participation in which we impose the assumption that it is relative exposure to *minorities* (blacks or Hispanics) at the school or neighborhood level that affects the relative test participation rate of black students, constraining the effects of the two types of peers to be identical. This specification leads to minority exposure effects that are roughly a weighted average of the black and Hispanic relative exposure effects in the more general model, with more of the weight on the black exposure effects. Like the more general model, this specification suggests that relative exposure to minority schoolmates has a weak positive effect on relative participation,

[32] The residual wage is computed as the MSA fixed effect in a regression of wages on years of education, indicators for high school dropout and college graduation, and a cubic in potential experience. The regression is computed separately for each race and for each gender, on samples of adults with resident children aged 0-18. We use the difference between black and white average residual wages among mothers and among fathers in the MSA.

whereas relative exposure to minority neighbors has a stronger (marginally significant) negative effect on participation rates.

Columns D and E of Table 4 present models for adjusted SAT scores that include the same control variables as the models in Columns E and F of Table 3. In both cases, the addition of the residential segregation measures causes the estimated effect of exposure to black schoolmates to fall to nearly zero, and causes the estimated effect of Hispanic schoolmates to become positive (but insignificant). In contrast, the neighborhood segregation effects are large, statistically significant, and relatively stable whether SAT-based background controls are excluded (Column D) or included (Column E). Column F extends the specification in column E by adding the residual wage gaps between black and white mothers and fathers in each MSA. The father's wage gap variable is statistically significant but has the "wrong sign,", perhaps reflecting a relationship with selection into SAT-taking, as indicated in column B. In any case, the inclusion of the wage gap variables has essentially no effect on the estimated segregation effects. Finally, in column G we report a model similar to the one in column F but restricting the relative exposure effects for blacks and Hispanics to be equal. Again, the estimated minority exposure effects lie between the estimated effects for exposure to blacks or Hispanics, but closer to the black effects. The restricted model suggests that exposure to minority schoolmates has no effect on the relative test scores of blacks, while exposure to minority neighbors has a strong negative effect.

We have estimated a wide variety of alternative specifications to probe the robustness of the conclusion that school segregation has little or no effect on relative test scores, while neighborhood segregation has a strong negative effect. Some of these alternative models are presented in Appendix Table 1. In one check, we include a dummy variable for cities from the three states with high fractions of Hispanic immigrants –

California, Florida, and Texas. This has no effect on the pattern of results seen in Table 4. In a second check, we add an additional measure of relative school segregation in the elementary schools in each city, with the idea that this may measure school-level peers during students' formative years better than does the high school segregation measure. When measures of relative exposure to minority classmates in both high schools and elementary schools are included together, both have very small (but quite imprecise) coefficient estimates. When only the elementary school segregation measure is included, it has a coefficient of 8.0 (standard error 29.1) – very similar to the coefficient estimate on relative exposure to minority schoolmates at the high school level in the model in column G of Table 4.

Finally, we estimated models that allow the effects of minority exposure to differ for black and white students. Specifically, we estimated a model with separate coefficients on the fractions of minority students in the average white and black students' schools, and on the fractions of minority neighbors in the average white and black residents' census tracts. This model (reported in column I of Appendix Table 1) yields estimated effects of exposure to minority schoolmates for blacks and whites that are both very close to 0 (6.7 and -6.6, respectively), an estimated effect of exposure to minority neighbors for black students that is negative and significant (-97.7 with a standard error of 30.4), and an estimated effect of exposure to minority neighbors for white students that is also negative but somewhat imprecise (-45.2 with a standard error of 91.5).[33] We obviously cannot reject the assumption that exposure to minority neighbors has a similar negative effect on both blacks and whites, and that neighborhood segregation therefore widens the black-white test score gap.

[33] The dependent variable is the black-white adjusted test score gap, so in the models with separate effects of minority exposure on black and white students the sign of the coefficient on white exposure is reversed.

C. Are the Segregation Effects Causal?

Table 4 essentially runs a "horse race" between school and residential segregation, and the residential measure wins handily. One interpretation of this finding is that school segregation has no causal effect, once the level of neighborhood segregation is taken into account. An alternative is that the estimated school or neighborhood segregation effects are biased relative to the true causal relationship by endogeneity of either school or neighborhood segregation. To assess this possibility, this section presents a series of instrumental variables (IV) estimates that isolate components of school or neighborhood segregation that are arguably exogenous. For simplicity, and based on the results in Table 4, we collapse the separate black and Hispanic exposure indices into measures of exposure to minority schoolmates or neighbors, as in Column G of that Table.

We begin by considering two instruments for school segregation. The first is the number of streams and rivers flowing through the metropolitan area, which has been shown to be a strong predictor of the number of school districts serving the area (Hoxby 2000; Rothstein 2005). Although many school districts operate programs to reduce the variation in racial and ethnic composition across high schools, there are few such inter-district programs. As a result, one would expect greater school segregation in metropolitan areas with more school districts, controlling for the degree of neighborhood segregation.[34] Our second instrument is a measure of the "bite" of court-ordered school desegregation programs implemented in the 1970s and early 1980s in many U.S. cities. We use Welch and Light's (1987) estimate of the change in the "dissimilarity index"—an alternative index of racial segregation—for a city's schools between the year prior to the city's major desegregation

[34] Hoxby (2000) argues that the number of districts has a causal effect on school productivity. To the extent that this effect raises black and white test scores equally, it is eliminated by our differencing strategy. In any case, Rothstein's (2005) re-analysis of Hoxby's data suggests there is no large or significant effect of inter-district competition on average test scores in a city. We use a measure similar to Rothstein's "total streams through [the] MSA," computed from the same USGS data but updated to 2000 MSA definitions.

plan and the last year of implementation.[35] This variable is only available for a subset of the MSAs in our sample.

IV models using these two instruments for school segregation (while controlling for neighborhood segregation) are reported in the upper panel of Table 5. As a point of departure the model in column A shows a baseline OLS specification. Column B presents a first stage regression of the degree of relative school segregation in a city on the number of streams flowing through the MSA, the degree of residential segregation, and the other controls. The streams variable has a positive, small but significant, effect on school segregation. Column C presents the IV estimate of the effect of school segregation on the racial gap in adjusted SAT scores, using streams as the instrument. Although this estimate is extremely imprecise, the point estimate is small, and offers little indication of any substantial upward bias in the baseline OLS estimate.

Columns D through F repeat the exercise using the desegregation instrument. Column D presents the OLS estimate for the subsample of 60 cities for which Welch and Light's (1987) data are available. The estimated neighborhood segregation effect is negligible in this subsample, though it is very imprecisely estimated. Column E presents the first stage model, using school desegregation orders as a predictor of school segregation. This model suggests that even after two decades or more the court orders continue to have an effect on observed measures of school segregation. Finally, the model in column F shows the IV estimate using the desegregation instrument. As before, the IV estimate is quite imprecise, but again there is no indication of an upward bias in OLS.

The second panel of Table 5 presents a set of models that treat residential

[35] This variable is set to zero for cities without a major desegregation plan. The Welch and Light measure pertains to districts, rather than to MSAs; we multiply their measure by the share of metropolitan enrollment that is in the relevant districts. MSAs with no districts in the Welch and Light sample are excluded.

segregation as endogenous. Our instrument for residential segregation is Cutler, Glaeser, and Vigdor's (1999) estimate of the isolation index in the city in 1980.[36] The idea of this instrument is to focus on the stable component of a city's residential segregation, under the assumption that the most likely sources of endogeneity will have had a greater effect on recent neighborhood segregation trends than past levels. In order to simplify the specification, and given the abundant evidence thus far that there is no effect of school segregation, we eliminate it from the model in this panel.

Columns A and B present OLS estimates of the effect of residential segregation on the black-white gap in adjusted SAT scores, first on our full sample and then on the subsample for which the 1980 measure is available. Column C presents the first stage regression of current residential segregation on the 1980 isolation index. Not surprisingly, there is a strong relationship, as residential segregation tends to evolve very slowly over time. Column D presents the IV estimate. There is little evidence of endogeneity bias in the OLS estimate; rather, the IV specification implies a substantially larger negative effect of residential segregation than is indicated by OLS.

We have also estimated a regression, not reported here, that enters both the 1980 and 2000 measures into our basic OLS specification for the adjusted SAT gap. Somewhat surprisingly—though consistent with the pattern of OLS and IV models presented in the lower panel of Table 5—the historical segregation measure seems to be a better predictor of current test score gaps than even the current segregation measure. Taken literally, this suggests that there is a positive relationship between black relative unobserved ability and recent changes in residential segregation. An alternative explanation for the larger effect of the 1980 segregation measure is measurement error. Our SAT-takers were born in the early-

[36] We are grateful to Jacob Vigdor for making this variable available. Sometimes, several 1980 MSAs correspond to a single current MSA; we then take the simple average of these.

to-mid-1980s, and neighborhood segregation in their formative years may have more closely resembled that seen in 1980 than it did our 2000 measure.[37] Moreover, tract boundaries in most cities were established in the 1950s, 1960s or 1970s around what were then homogenous neighborhoods. To the extent that neighborhood boundaries have migrated since then without changes in the underlying degree of segregation, measures of current segregation (which use the obsolete neighborhood boundaries as encoded in tracts) may be worse measures of true residential segregation than are the 1980 measures.

A second explanation might be that the current city-wide residential segregation index is a poor measure of the exposure of upper-middle-class families—from which SAT-takers disproportionately come—to minority neighbors, and that the 1980 index better proxies for this. Columns G and H of Appendix Table 1 report two specifications designed to investigate this. We compute an alternative current residential segregation index that measures the relative fraction minority in the census tracts where blacks and whites with a BA degree or more live. This correlates highly with the original current segregation index, and yields qualitatively similar results when used in its place; if anything, the results suggest that the original index better predicts relative (selection-adjusted) SAT scores.

D. Selection into SAT-taking

A potential concern with the results so far is that despite our efforts to adjust for selection biases—restricting the sample to cities in high-SAT-participation states, re-weighting the data from different high schools to offset differences in school-level participation, and controlling for the average inverse Mill's ratio terms associated with each

[37] In a supplementary table, available on request, we have explored several alternative ways of measuring segregation in a city (such as focusing on inner-city tracts only). These alternatives all perform worse than the simple citywide average used in the paper.

high school—the models may be biased by selective participation, leading to a faulty conclusion about the relative effects of school and neighborhood segregation. To probe the robustness of our results, Table 6 presents estimates of our basic (OLS) specifications that omit the Mill's ratio control function and that do not use our re-weighting procedure in the computation of city-level adjusted test score gaps. Beyond these differences, specifications A and B of Table 6 are identical to the ones in column E of Tables 3 and 4, respectively, while the model in column C of Table 6 restricts the black and Hispanic exposure effects to be equal. As in the previous models, the simpler unadjusted models show a significant effect of relative exposure to black schoolmates that falls in size and becomes statistically insignificant once we control for residential segregation. The residential segregation coefficients from the unadjusted models are negative, as in the adjusted models, but somewhat smaller in absolute value, perhaps reflecting the impact of attenuation biases arising from selective participation.[38]

A second and arguably more persuasive way to evaluate the impact of selective test participation is to examine models for black-white relative achievement based on outcomes for a random sample of youths. We use the 2000 Census 5-percent micro samples to construct two outcome measures for 16-24 year olds in each city: the fraction either employed or in school (an indicator of gainful activity), and the fraction who either are currently enrolled or have completed high school (an indicator of education attainment). A limitation of the Census data is that there is no family background information for children who are no longer living with their parents.[39] Consequently, we make no individual-level adjustments for family background. Instead, we regress the black-white difference for each

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[38] We have also explored other forms of correction, including artificially trimming the data to retain the same fraction of the high school population in each city. Our basic results of large negative effects of residential segregation and essentially zero effects of non-residential segregation have held up in every specification.

[39] To insulate against bias from endogenous mobility of young people who have left their parents' homes, we assign individuals to the MSA where they lived in 1995, when they were aged 11-19.

outcome measure on our school and residential segregation measures and the same Census-based family background measures used in Tables 3-6.

Table 7 presents a series of models for each of the two outcomes, fit to a sample of 234 MSAs with at least 50 students of each race in the PUMS samples. The models in the upper panel look at the black-white gap in the fraction of youths who are employed or in school, while the lower panel presents models for the gap in the fraction who are enrolled or have obtained a high school degree. The models in columns A-C include only our school segregation measures, while the models in columns D-F include school and neighborhood segregation measures. Within each group, we begin with specifications that include no other controls (columns A and D), then add the basic city controls included in all our previous models (columns B and E), and finally add Census-based controls for family background differences between blacks and whites in each city (columns C and F).

Beginning with the models in columns A and B, note that with no controls, or only a limited set of city controls, there appears to be a negative relationship between black youths' relative outcomes in a city and their relative exposure to black (and perhaps Hispanic) schoolmates. As shown in the parallel models in columns E and F, however, adding controls for residential segregation essentially eliminates the effect of relative exposure to black schoolmates, and suggests instead that relative exposure to black neighbors is a key determinant of black youth's relative outcomes. These results are remarkably similar to our findings for black relative test scores, and suggest that the test score findings cannot be attributed to statistical problems arising from selective SAT participation.

Examination of the models in columns C and F suggests that inferences about the effects of relative segregation on employment or educational attainment are sensitive to the set of background control variables added to the model.[40] In particular, once the relative background variables we use in Tables 3-6 are added, the estimated impacts of school segregation on its own, or school and neighborhood segregation taken together, fall in magnitude and become insignificant. By contrast, the models in Table 4 show robust negative effects of relative exposure to minority neighbors on black-white relative test scores. One plausible explanation for the difference is that neighborhood segregation has smaller effects on basic achievement outcomes (like being in gainful activity or completing high school) than on higher-level achievement outcomes (like college entry test scores). Unfortunately, however, the Census outcome models have limited power against reasonable effect sizes, so it is difficult to say anything conclusive about this.

V. POSSIBLE CONFOUNDERS OF SCHOOL-LEVEL SEGREGATION EFFECTS

The results in Tables 4-7 suggest that relative exposure to black neighbors has a negative impact on black relative achievement, whereas relative exposure to black schoolmates has little or no effect. In this section, we explore three explanations for the somewhat puzzling lack of any school-level exposure effect. The first is that the relative quality of schools attended by black students is correlated with neighborhood and school segregation in such a way as to offset a true negative effect of school racial composition. A second is that unobserved differences in school-level peer characteristics bias the effects of schoolmates' racial composition. Third, classroom peer groups may be what matter for student performance, and cities with more aggressive school integration programs may also have more segregation within schools.[41] We investigate each of these hypotheses in turn.

[40] The controls in Column E are similar to those used by Cutler and Glaeser (1997), who find large and significant effects of residential segregation on black relative outcomes in a somewhat different specification.
[41] This mechanism is consistent with anecdotal evidence suggesting that districts with stronger desegregation

A. Relative School Quality

Our first candidate explanation for the contrasting effects of neighborhood and school segregation is that either or both types of segregation may be related to the gap in school quality between the schools attended by the black and white students in a city. Unfortunately, there are few sources of information on school quality that can be used to assess this hypothesis. The only school-level measure that is universally available (from the Common Core of Data) is the number of full-time-equivalent (FTE) teachers. We use this source to compute the number of teachers per student at public schools attended by white and black students in each MSA.[42] Measures of spending are available only at the district level. Using the CCD Local Education Agency Finance Survey (also known as the F-33 portion of the Census of Governments) we compute expenditures per pupil in districts attended by white and black students in each MSA.[43] For information on other dimensions of school quality we turn to the Schools and Staffing Survey (SASS), which has information on the qualifications, experience, and characteristics of a national sample of teachers that can be matched to the racial composition of the schools in which they teach.

Table 8 presents models with the same explanatory variables as were used to explain

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programs often create special programs to attract white students to high-minority schools (Clotfelter, Ladd et al. 2003; Clotfelter 2004; Eyler, Cook et al. 1983), and that these programs may reduce exposure. For example, the federal district court judge's opinion in *People Who Care v. Rockford Board of Education*, 851 F. Supp. 905 (1993) states: "The court finds that the ability grouping and tracking practices of the Rockford School District (hereinafter 'RSD') did not represent a trustworthy enactment of any academically acceptable theory or practice. The RSD tracking practices skewed enrollment in favor of whites and to the disadvantage of minority students. The court finds that it was the policy of the RSD to use tracking to intentionally segregate white students from minority students…." (p. 940)

42 This calculation includes all grades, even though class sizes are typically smaller in elementary schools, because FTEs are only available at the school level and the separation of grades into elementary, middle, and high schools varies somewhat across metropolitan areas. For this reason, we use teachers per student rather than the inverse (which has a more natural interpretation as the average class size), as the enrollment-weighted average of the former ratio for each race is insensitive to the way that heterogeneous grades are distributed among schools.

43 Unfortunately, if resource allocations are not equal across schools in each district, this is an imperfect measure of the actual spending at black and white students' schools.

black-white test score gaps in Table 4, including city-level controls and black-white differences in family background characteristics.[44] In the first two columns, the dependent variables are the black-white gap in per pupil expenditures and in teachers per pupil in the MSA. It appears from these columns that there is little relationship between segregation and spending, but that school and neighborhood segregation have some impact on class size. Specifically, residential segregation is associated with larger classes at black students' schools (though this is only significant for the Hispanic exposure measure), while school segregation is associated with smaller classes for blacks. To the extent that smaller classes (i.e., a higher ratio of teachers to students) raise achievement, these findings may help explain our estimated school and neighborhood segregation effects, since they suggest that black students have lower quality schooling in cities with more segregated neighborhoods but higher quality schools in cities with more segregated schools. It should be noted, however, that the size of the effect is modest: the coefficient of 1.15 in the first row of column B implies that moving from fully integrated to fully segregated schools would raise the teacher-pupil ratio by 0.01, implying a class size reduction of about 15% at the mean. Most recent studies suggest the effect of such a change would be modest.[45] Moreover, we suspect some of the "effect" of relative segregation on class size may reflect black students' disproportionate likelihood of being assigned to special education programs (Donovan and Cross, 2002), which have smaller classes but would not be expected to raise SAT scores.

Columns C-F of Table 8 report similar models for gaps in average teacher

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[44] Since the resource measures do not distinguish among grades and are limited to public schools, the school segregation measures used in the models in columns A and B of Table 8 are computed over public school students in all grades.

[45] For example, Krueger (1999) finds that the STAR experiment, which raised teachers per pupil by about 40-50 percent, had an effect on third grade test scores of about 0.2 standard deviations. Using the effect size in the text, Krueger's estimate would imply an effect of school segregation on SAT scores of under 20 points. This might be understated somewhat, given Krueger and Whitmore's (2002) conclusion that black students are more sensitive to class size than are whites, but 40 points seems like a reasonable upper bound.

characteristics, estimated from the SASS, between schools attended by black and white students. The model in column C shows that black students have a substantially lower relative fraction of white teachers in cities with greater school segregation. Interestingly, there is no corresponding effect of neighborhood segregation. The models for the gap in average salaries and experience between the teachers of black and white students (columns D and E) are relatively noisy but show no significant segregation effects. Finally, the model in column F shows that both school and neighborhood segregation lead to increases in the relative fraction of black students' teachers who have undergraduate degrees in education, with a larger effect for neighborhood segregation. Assuming that the fraction of teachers with an education major is a negative quality indicator, this could result in some overstatement of the relative causal effect of neighborhood segregation, though we suspect any such effect is small. Overall, we interpret the results in columns A-F of Table 8 as suggesting that unmeasured school quality effects are an unlikely explanation for our finding that neighborhood segregation affects black relative achievement while school segregation does not.

B. Unmeasured Schoolmate Characteristics

As we noted in Section II, our data sources do not allow us to estimate the difference between the average characteristics—other than racial composition—of school-level peer groups of black and white students in a city. Our suspicion is that omission of other characteristics will tend to lead to an *overstatement* of the negative effect of exposure to minority schoolmates, since schools with more black students tend to have more students with disadvantaged family backgrounds. To provide some evidence on this conjecture, we used data from the CCD to estimate the black-white gap in the average fraction of schoolmates receiving free or reduced price lunches. Column G of Table 8 presents coefficient estimates from a model relating this measure of relative peer group economic status to our measures of school and neighborhood segregation.[46] As we expected, there is a strong positive correlation with exposure to both black and Hispanic schoolmates. On the other hand, there is essentially no relationship with residential segregation. To the extent that the presence of lower-income peers depresses academic achievement, these findings suggest that the absence of data on the characteristics of school-based peers would, if anything, lead us to find an effect of school segregation, but would not lead to any magnification of the effect of neighborhood segregation. Thus, missing data on school-level peers seems like an unlikely explanation for our main findings.

C. Across-school segregation and within-school exposure

Our final candidate explanation is that student achievement is primarily affected by classroom-level rather than school-level peers, and that variation across cities in the relative exposure of black and white students to black *schoolmates* (conditional on residential segregation) is only weakly correlated with the relative exposure to black *classmates*. This would lead us to find little effect of non-residential school segregation, as it would have little signal for the classroom-level segregation that would be the relevant measure. This hypothesis is difficult to test directly, as there are no national data on the racial composition of high school classrooms. We therefore focus on an indirect test, based on the covariance between the prevalence of ability tracking and the residential and non-residential components of school segregation.

[46] We opt not to include the free lunch measure as a control in our SAT score models because we suspect that it is a less reliable measure of school poverty at the secondary level than for elementary schools, where take-up rates are higher. In Table 8, we measure free lunch rates over all grades.

We use data on course enrollment patterns from the SAT data set to measure ability tracking. Specifically, SAT-takers are asked whether they have taken honors courses and whether they intend to claim advanced placement (AP) credit or course exemptions in college on the basis of high school work. Columns A and B of Table 9 present models for the fraction of students who indicated that they had taken honors courses in math or English, respectively, while Columns C and D present models for the fraction of students who intended to claim college-level credit in any subject (column C) or in math or English (column D). As in earlier tables, we combine our measures of exposure to blacks and Hispanics into indexes of exposure to minority schoolmates or neighbors, though estimates from models that separate the two groups are very similar.

In Panels A and B we present estimates of the relationships between the school and neighborhood segregation measures and the black and white means of the course-taking variables. The estimates in Panel A show no significant relationship between either school or neighborhood segregation and black course-taking. The estimates in Panel B, by comparison, show relatively strong negative impacts of segregation on honors and AP participation by whites, many of which are at the margin of significance. To interpret these impacts, note that a rise in the relative exposure of blacks to minority schoolmates implies that whites are relatively less exposed to minorities. Thus, a negative coefficient means that white students are more likely to take honors and AP classes in cities with more integrated schools and neighborhoods. Finally, Panel C reports estimates for the black-white difference in honors participation at the city level. Increased school segregation is associated with large positive effects on the black-white gap in honors course taking and in AP participation. Increases in neighborhood segregation also tend to have positive effects, although the coefficients are smaller and uniformly insignificant.

Though relative participation in honors and AP courses is a limited measure of within-school segregation, the results in Table 9 seem to offer fairly strong support for the within-school segregation hypothesis. In particular, holding constant neighborhood segregation, white students are more likely to participate in "high track" courses when schools are more integrated, presumably limiting the classroom-level exposure of blacks to whites.[47]

VI. Do Neighborhood Segregation Effects Reflect Race or Other Factors?

As a final interpretative exercise, we examine the source of the neighborhood "peer effects" that are implied by our estimates: Are black neighbors inherently bad for student performance, or do our results reflect other neighborhood characteristics that are correlated with race? To explore this, we add to our basic specification controls for the relative exposure of white and black residents to alternative neighborhood characteristics, such as the poverty rate. Without these controls, the measured effect of relative exposure to black or Hispanic neighbors presumably combines the direct effect of racial composition with the effects of other relative neighborhood quality variables that are correlated with racial composition.

Table 10 reports the results. Again, for simplicity we have combined relative exposure to black and Hispanic schoolmates or neighbors into measures of exposure to minority schoolmates and neighbors. Column A presents our baseline specification, without

[47] We have also estimated models for the tracking measures that separate out the components of school segregation attributable to court-ordered desegregation. Standard errors are large, but the results indicate that, if anything, court-ordered desegregation has larger effects on tracking than does the residual component. Also, though we focus in Table 9 on tracking in secondary grades, an analysis of data on kindergarten classroom composition from the Early Childhood Longitudinal Survey suggests that cities with more non-residential school segregation have schools that are, at the kindergarten level, significantly *more* internally segregated than are schools in cities with less school segregation.

additional neighborhood controls. Column B adds a control for the difference in the log of per capita income between neighborhoods in which black and white families reside. This variable has a significant positive effect, indicating that in cities where blacks' neighborhoods have higher relative incomes, black relative SAT scores are higher. Moreover, the addition of this variable has a notable effect on the estimated residential segregation coefficient, reducing the effect of exposure to minority neighbors by about 40%. The next four columns explore alternative controls for differences in the economic status of black and white neighborhoods, while the last includes all of the neighborhood measures together. None of the other neighborhood variables is a significant predictor of the black-white test score gap. However, when all are included (in column G), the effect of relative exposure to black neighbors is about 25% smaller than we obtained when the neighborhood measures are excluded, and is only marginally significant.

We interpret the pattern of coefficients in Table 10 as suggesting that the measured neighborhood segregation effects in models that exclude other relative neighborhood characteristics overstate the effects of minority exposure *per se*. Indeed, looking at the specifications in columns B and G, it appears that relative exposure to low income neighbors has as important an effect as does relative exposure to minority neighbors.

VII. SUMMARY AND CONCLUSIONS

In this paper we present new evidence on the effects of racial segregation on the relative achievement of black students. Building from a model in which the racial composition of school and neighborhood peer groups exerts a causal effect on student achievement, we show that the black-white achievement *gap* in a city will vary with the relative segregation of schools and neighborhoods in the city. The model also suggests that in measuring the effects of racial segregation it is important to control for relative exposure of black and white students to other characteristics of school and neighborhood peer groups, such as family income. Otherwise, these differences will tend to lead to an overstatement of the effects of race *per se*.

Our main empirical evidence is based on SAT outcomes for all the black test takers and one-quarter of the white test takers in the 1998-2001 test cohorts. We match test-takers to information on the racial composition of their high schools and to an extensive set of family background characteristics of black and white students in different cities. We use data from the summary files of the 2000 Census to construct estimates of the relative exposure of white and black students in a city to a variety of neighborhood characteristics, including racial/ethnic composition, income, and family structure. To address concerns about potential selectivity biases in the SAT outcomes, we also use 2000 Census micro data to construct measures of the relative achievement of black and white youth in different metropolitan areas.

Without controlling for neighborhood segregation, we observe that school segregation has a negative effect on black relative test scores and on achievement measures from the Census. In models that include both school and neighborhood segregation, however, the effects of relative exposure to black and Hispanic schoolmates are uniformly small and statistically insignificant, whereas the effects of relative exposure to black and Hispanic neighbors are negative. Probes into possible explanations for the absence of school segregation effects, including instrumental variables estimates and assessments of correlated differences in unobserved school or peer quality, give no indication that our estimates are biased in a way that would obscure negative effects of school segregation. Finally, in models that include school segregation, neighborhood segregation, and measures of the relative exposure of blacks to other characteristics of their neighbors, even the neighborhood segregation effects are diminished.

Taken as a whole, our results suggest that concerns over the racial isolation of black youth may be overstated. Consistent with the findings of Cutler and Glaeser (1997), neighborhoods appear to matter for student achievement. As suggested by Wilson (1987), however, race *per se* may not be the primary source of these effects: rather, it seems to be exposure to more economically successful neighbors. Moreover, holding constant neighborhood characteristics, the racial composition of schools seems to have little effect on black relative achievement. Given recent trends toward ending formal desegregation programs in many cities, this may be good news.

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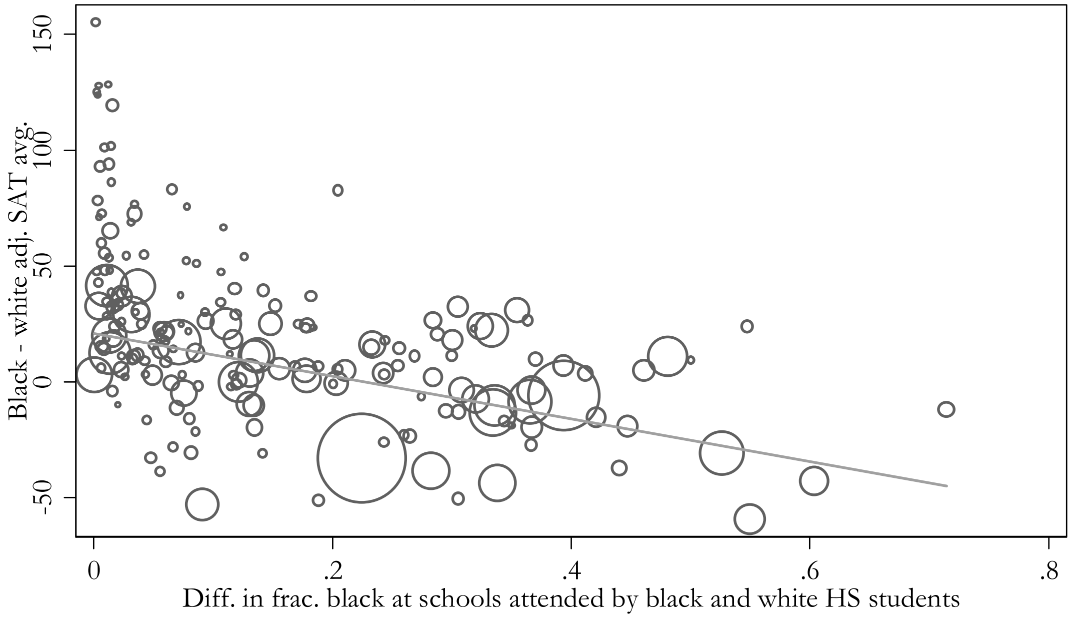
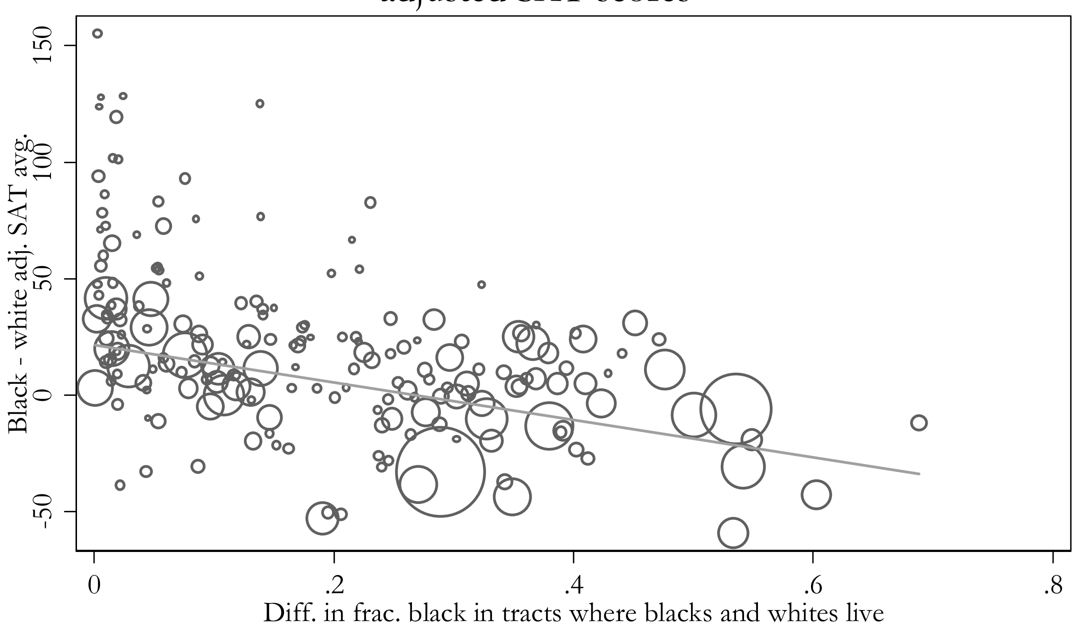
Figure 1. School segregation and black-white gaps in adjusted SAT scores



Notes: Sample is metropolitan areas in SAT states. Circle sizes are proportional to the sampling error variance in MSA black-white gaps (see text for details). Line is the weighted least squares regression line.

Figure 2. Residential segregation and black-white gaps in adjusted SAT scores



Notes: Sample is metropolitan areas in SAT states. Circle sizes are proportional to the sampling error variance in MSA black-white gaps (see text for details). Line is the weighted least squares regression line.

Table 1: Residential and school segregation in representative metropolitan areas

Name	Residential Fraction black in: Blacks' tracts (A)	Whites' tracts (B)	Diff. (C)	High school students Fraction black in: Blacks' schools (D)	Whites' schools (E)	Diff. (F)	SAT State? (G)	Avg. SAT score Blacks (H)	Whites (I)	Diff. (J)
Least Segregated Cities										
Missoula, MT MSA	0.6%	0.5%	0.1%	0.4%	0.3%	0.1%	N			
Laredo, TX MSA	0.3%	0.2%	0.1%	0.1%	0.1%	0.1%	Y	743	962	-218
Bismarck, ND MSA	0.5%	0.4%	0.1%	0.2%	0.2%	0.1%	N			
Provo-Orem, UT MSA	0.6%	0.4%	0.2%	0.5%	0.2%	0.2%	N			
Brownsville-Harlingen-San Benito, TX	0.6%	0.4%	0.2%	1.8%	1.3%	0.4%	Y	808	998	-190
Least segregated cities with at least 10% blacks										
Jacksonville, NC MSA	25.9%	17.1%	8.8%	32.5%	23.9%	8.6%	Y	886	1014	-128
Lawton, OK MSA	27.5%	17.6%	9.9%	29.5%	22.0%	7.5%	N			
Dover, DE MSA	29.4%	18.7%	10.7%	29.0%	22.3%	6.6%	Y	843	1012	-169
Killeen-Temple, TX MSA	29.7%	17.5%	12.3%	32.5%	18.2%	14.2%	Y	869	1021	-152
Charlottesville, VA MSA	25.7%	12.5%	13.1%	27.6%	16.1%	11.5%	Y	858	1073	-215
Most segregated cities										
Birmingham, AL MSA	69.4%	12.8%	56.7%	77.8%	14.0%	63.7%	N			
St. Louis, MO-IL MSA	64.5%	7.8%	56.7%	59.8%	11.3%	48.4%	N			
Monroe, LA MSA	71.6%	13.9%	57.7%	72.2%	20.0%	52.2%	N			
Newark, NJ PMSA	66.7%	6.4%	60.3%	68.1%	7.7%	60.4%	Y	810	1061	-252
Milwaukee-Waukesha, WI PMSA	67.2%	5.3%	61.9%	61.8%	7.4%	54.5%	N			
Flint, MI PMSA	70.1%	7.5%	62.6%	69.8%	7.6%	62.2%	N			
Cleveland-Lorain-Elyria, OH PMSA	70.4%	6.3%	64.0%	69.3%	7.7%	61.7%	N			
Chicago, IL PMSA	72.3%	5.6%	66.7%	65.7%	7.2%	58.5%	N			
Gary, IN PMSA	73.6%	4.8%	68.8%	76.3%	4.8%	71.4%	Y	798	993	-195
Detroit, MI PMSA	79.0%	5.7%	73.3%	80.9%	5.2%	75.7%	N			

Notes: Segregation rankings are by difference in fraction black between blacks' and whites' census tracts, as in the third column, among 331 MSAs and PMSAs.

Table 2. Summary statistics for cities in the SAT sample

	All Cities		In SAT states		Not in SAT states	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
	(A)	**(B)**	**(C)**	**(D)**	**(E)**	**(F)**
N	331		189		142	
Population (millions)	2.856	3.010	3.042	3.168	2.412	2.552
Fraction black	0.12	0.09	0.11	0.08	0.14	0.10
Fraction Hispanic	0.21	0.21	0.25	0.23	0.09	0.10
log(Mean HH income)	10.98	0.19	10.99	0.20	10.96	0.16
Segregation (Black fraction black - white fraction black)						
Residential (Tract)	0.29	0.20	0.26	0.18	0.36	0.24
High schools	0.25	0.19	0.22	0.16	0.33	0.23
SAT-takers' schools (reweighted)			0.21	0.15		
SAT-taking rate						
All students	0.32	0.15	0.38	0.10	0.16	0.12
White students	0.36	0.16	0.43	0.11	0.18	0.12
Black students	0.25	0.13	0.31	0.09	0.10	0.10
SAT-takers						
Avg. SAT	1033.5	71.2	999.5	45.7	1114.8	53.0
Black-white avg. SAT	-193.3	36.5	-194.0	34.3	-191.6	41.3

Notes: All summary statistics are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$, where N_w and N_b are the number of white and black residents of the MSA, respectively. Average SATs and black-white SAT differences use SAT sampling weights within cities.

Table 3. Basic estimates of school segregation's effect on black-white differences in SAT participation and residual scores

	B-W participation rate		B-W avg. residual SAT score			
	(A)	(B)	(C)	(D)	(E)	(F)
Black-white difference: Fr. black in HS students' schools	-0.21	-0.02	-120.0	-98.4	-55.1	-49.0
	(0.07)	(0.06)	(29.7)	(28.6)	(22.2)	(25.1)
Black-white difference: Fr. Hispanic in HS students' schools	-0.19	-0.05	-68.5	-82.7	-28.0	-14.5
	(0.12)	(0.06)	(23.7)	(31.9)	(28.6)	(27.6)
Fr. black "main effect:" Fr. black in white & black HS students' schools	0.03	-0.08	-7.3	5.6	8.5	10.2
	(0.07)	(0.08)	(32.2)	(28.6)	(28.2)	(29.1)
Fr. Hispanic "main effect:" Fr. Hispanic in white & black HS students' schools	0.37	0.22	-5.9	27.0	1.1	1.6
	(0.12)	(0.07)	(12.8)	(21.6)	(25.1)	(20.7)
B-W background index (SAT-takers)				1.35	1.05	2.14
				(0.31)	(0.23)	(0.92)
B-W fraction of kids living with one parent (census)		-0.43			-69.4	-105.5
		(0.18)			(65.6)	(60.1)
B-W fraction of kids living with neither parent (census)		-0.36			-96.3	-118.0
		(0.22)			(73.8)	(72.9)
B-W: Mothers some college (census)		0.20			23.1	40.8
		(0.11)			(47.9)	(43.5)
B-W: Mothers BA+ (census)		0.04			66.6	62.8
		(0.16)			(56.3)	(56.8)
B-W: Fathers some college (census)		0.18			-40.4	-45.2
		(0.09)			(55.6)	(44.0)
B-W: Fathers BA+ (census)		0.22			-13.7	-23.9
		(0.17)			(46.0)	(49.3)
B-W employment rate of mothers (census)		-0.05			-14.0	-17.5
		(0.12)			(46.3)	(51.5)
B-W median family income (census; \$10,000s)		0.004			1.5	0.9
		(0.007)			(2.2)	(2.4)
B-W child poverty rate (census)		-0.33			-100.9	-52.8
		(0.13)			(57.4)	(57.0)
B-W inverse Mills ratio			-46.2	-26.2	24.3	22.4
			(14.3)	(14.2)	(14.6)	(14.8)
B-W: Fathers some college (SAT-takers)						57.8
						(57.5)
B-W: Fathers BA+ (SAT-takers)						-159.3
						(70.0)
B-W: Mothers some college (SAT-takers)						-156.8
						(55.3)
B-W: Mothers BA+ (SAT-takers)						108.4
						(70.2)
B-W family income (SAT-takers; in SAT points)						0.36
						(2.13)
N	185	185	185	185	185	185
R-squared	0.73	0.84	0.59	0.65	0.74	0.77
p-value, B-W fr. Black=B-W fr. Hispanic	0.86	0.64	0.13	0.72	0.42	0.29
p-value, B-W fr. Black=B-W fr. Hispanic=0	0.01	0.70	0.00	0.00	0.04	0.15

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls for the log of the city population and for the city land area, fraction with some college and BAs, log mean HH income, gini coefficient, and census division effects. City-level black-white differences in residual SATs (columns C-F) are computed over SAT-taker data that are re-weighted using school-by-race participation rates; see text for details. All standard errors are clustered on the CMSA.

Table 4. Residential and school segregation effects on black-white differences in SAT participation and residual scores

	B-W partic. rate			B-W avg. residual SAT score			
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Black-white difference: Fr. black in HS students' schools	0.03	0.05		-3.1	0.4	-8.6	
	(0.08)	(0.08)		(29.4)	(32.9)	(31.5)	
Black-white difference: Fr. Hispanic in HS students' schools	0.30	0.29		80.9	79.2	94.6	
	(0.14)	(0.13)		(63.4)	(64.3)	(58.5)	
Black-white difference: Fr. minority in HS students' schools			0.10				10.4
			(0.08)				(29.2)
Black-white difference: Fr. Black in residents' census tracts	-0.07	-0.08		-88.5	-81.4	-73.3	
	(0.09)	(0.09)		(27.7)	(30.5)	(28.7)	
Black-white difference: Fr. Hispanic in residents' census tracts	-0.36	-0.37		-137.7	-120.7	-126.3	
	(0.13)	(0.13)		(56.1)	(61.0)	(55.8)	
Black-white difference: Fr. minority in residents' census tracts			-0.17				-78.9
			(0.08)				(28.0)
B-W inverse Mills ratio				30.3	30.7	28.3	25.4
				(15.1)	(16.1)	(15.6)	(14.8)
B-W residual wage gap in MSA: Mothers		0.00	0.00			-5.9	-14.7
		(0.07)	(0.06)			(26.9)	(25.7)
B-W residual wage gap in MSA: Fathers		0.08	0.08			-47.7	-41.2
		(0.04)	(0.04)			(17.3)	(16.2)
Census B-W bkgd. controls	y	y	y	y	y	y	y
B-W background index (SAT-takers)	n	n	n	y	y	y	y
Additional SAT-taker B-W bkgd. controls	n	n	n	n	y	y	y
N	185	185	185	185	185	185	185
R-squared	0.81	0.81	0.81	0.76	0.78	0.79	0.79
p-value, school segregation effect=0	0.10	0.08	0.19	0.44	0.46	0.24	0.72
p-value, residential segregation effect=0	0.02	0.02	0.05	0.00	0.01	0.01	0.01

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls for the log of the city population and for the city land area, fraction with some college and BAs, log mean HH income, gini coefficient, and census division effects. Census background controls are those in Column B of Table 3; additional SAT-taker controls are those in Column F of that table. All standard errors are clustered on the CMSA.

Table 5. Instrumental variables estimates

Panel A: School segregation	Streams as instrument			Welch & Light subsample		
	OLS	First stage	IV	OLS	First stage	IV
	(A)	(B)	(C)	(D)	(E)	(F)
Black-white difference: Fr. minority in	-92.0	0.93	-78.1	-9.3	0.97	-57.5
residents' census tracts	(26.1)	(0.07)	(118.6)	(47.0)	(0.13)	(111.7)
Black-white difference: Fr. minority in	7.9		-7.3	-34.4		13.5
SAT-takers' schools	(26.2)		(135.6)	(45.1)		(115.8)
Number of streams through MSA (/1000)		0.090				
		(0.030)				
Change in dissimilarity index induced by					0.16	
major desegregation plans (/100)					(0.06)	
N	185	185	185	60	60	60
F statistic, exclusion of instruments		9.1			8.7	

Panel B: Residential segregation	Full sample	1980 subsample		
	OLS	OLS	First stage	IV
	(A)	(B)	(C)	(D)
Black-white difference: Fr. minority in	-84.8	-71.6		-166.8
residents' census tracts	(19.0)	(19.5)		(51.4)
1980 Isolation index			0.39	
			(0.05)	
N	185	158	158	158
F statistic, exclusion of instruments			50.4	

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls from column G of Table 4. All standard errors are clustered on the CMSA.

Table 6: Estimates for SAT averages unadjusted for participation rates

	(A)	(B)	(C)
Black-white difference: Fr. black in SAT takers' schools	-42.3	-21.3	
	(21.3)	(25.9)	
Black-white difference: Fr. Hispanic in SAT-takers' schools	-17.5	72.6	
	(25.9)	(48.9)	
Black-white difference: Fr. minority in SAT-takers' schools			-3.5
			(20.9)
Black-white difference: Fr. black in residents' census tracts		-38.1	
		(26.0)	
Black-white difference: Fr. Hispanic in residents' census tracts		-105.9	
		(39.9)	
Black-white difference: Fr. minority in residents' census tracts			-58.5
			(25.4)
N	185	185	185
R-squared	0.78	0.79	0.78
p-value, school segregation effect=0	0.095	0.276	0.867
p-value, residential segregation effect=0		0.029	0.023

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable is the difference between black and white means of un-reweighted adjusted SAT scores. Control variables are as in Column F of Table 4, except that the inverse Mill's ratio term is excluded and the un-reweighted SAT-taker data are used to compute the black-white difference in family background among SAT-takers. All standard errors are clustered on the CMSA.

Table 7. Residential and school segregation effects on black-white differences in alternative outcome measures, measured from Census data

Dependent variable: B-W gap in percentage who are employed or in school	(A)	(B)	(C)	(D)	(E)	(F)
B-W fr. Black in HS students' schools	-10.4	-6.0	0.8	4.7	5.1	2.6
	(2.4)	(3.4)	(2.1)	(3.9)	(4.2)	(3.2)
B-W fr. Hispanic in HS students' schools	-0.3	-2.9	2.3	-4.2	-12.3	-8.4
	(3.2)	(4.3)	(4.3)	(9.9)	(13.3)	(8.2)
B-W fr. Black in residents' census tracts				-17.6	-18.0	-3.0
				(4.5)	(4.5)	(3.8)
B-W fr. Hispanic in residents' census tracts				4.8	7.6	11.0
				(11.1)	(13.7)	(8.5)
Basic city controls	n	y	y	n	y	y
B-W gaps in observables	n	n	y	n	n	y
N	234	234	234	234	234	234
R-squared	0.21	0.37	0.58	0.30	0.41	0.59
p-value, school segregation effect=0	0.00	0.21	0.84	0.40	0.28	0.40
p-value, resid. segregation effect=0				0.00	0.00	0.30

Dependent variable: B-W gap in percentage who have finished HS or are in school	(A)	(B)	(C)	(D)	(E)	(F)
B-W fr. Black in HS students' schools	-10.2	-8.1	-1.5	1.6	0.9	-1.1
	(1.8)	(3.0)	(1.9)	(3.8)	(3.9)	(3.1)
B-W fr. Hispanic in HS students' schools	-2.7	-8.8	-4.5	-13.3	-13.9	-12.1
	(5.1)	(5.1)	(4.8)	(8.8)	(12.7)	(8.7)
B-W fr. Black in residents' census tracts				-13.2	-14.5	-0.6
				(3.7)	(5.4)	(4.2)
B-W fr. Hispanic in residents' census tracts				11.9	3.6	8.2
				(9.5)	(11.8)	(9.3)
Basic city controls	n	y	y	n	y	y
B-W gaps in observables	n	n	y	n	n	y
N	234	234	234	234	234	234
R-squared	0.18	0.34	0.55	0.23	0.36	0.55
p-value, school segregation effect=0	0.00	0.02	0.54	0.21	0.51	0.38
p-value, resid. segregation effect=0				0.00	0.03	0.67

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variables range in principle from -100 to 100, and have means (S.D.s) of -11.7 (3.8) and -6.9 (3.8), respectively. Each is measured over 16-24 year olds in the 2000 census, assigned to the metropolitan area where they lived in 1995. Sample excludes MSAs with fewer than 50 blacks or 50 whites in this sample. Control variables are those in column B of Table 3. All standard errors are clustered on the CMSA.

Table 8. Estimates of residential and school segregation's effects on black-white differences in school resources and teacher characteristics

	Resources (CCD)		Teacher characteristics (SASS)				School demographics (CCD)
	PP Expenditures ($1,000s)	Teacher / pupil ratio * 100	Fraction white	Avg. salary ($1,000s)	Avg. exper.	BA: Educ. Major	Fraction free lunch in school
	(A)	(B)	(C)	(D)	(E)	(F)	(G)
B-W fr. Black in students' schools	1.03	1.15	-0.54	-3.56	5.09	0.21	0.44
	(1.01)	(0.44)	(0.15)	(5.43)	(3.94)	(0.11)	(0.06)
B-W fr. Hispanic in students' schools	-0.19	0.66	-0.67	-3.26	2.89	-0.07	0.61
	(1.93)	(0.99)	(0.29)	(13.59)	(10.72)	(0.34)	(0.11)
B-W fr. Black in residents' census tracts	0.18	-0.82	0.10	0.01	2.38	0.54	-0.11
	(1.17)	(0.51)	(0.20)	(7.44)	(5.20)	(0.17)	(0.08)
B-W fr. Hispanic in residents' census tracts	-0.33	-1.89	0.11	5.23	1.82	-0.12	-0.15
	(1.95)	(0.87)	(0.34)	(12.55)	(9.15)	(0.38)	(0.11)
N	323	305	320	320	320	320	300
R-squared	0.36	0.38	0.62	0.12	0.15	0.18	0.87
p-value, school segreg. effect=0	0.60	0.03	0.00	0.79	0.41	0.16	0.00
p-value, resid. segreg. effect=0	0.97	0.06	0.86	0.91	0.87	0.00	0.07

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Dependent variable in each column is the estimated difference between the average of the indicated variable in black students' schools (districts in col. A) and that in white students' schools. School segregation measures are computed over enrollment in all grades, and over public schools in Columns A, B, and G. All columns include controls from column F of Table 4, minus the SAT-taker background index and inverse Mill's ratio terms. All standard errors are clustered on the CMSA.

Table 9. Residential and school segregation effects on measures of honors course-taking among SAT-takers

	=100 if honors courses in		=100 if plan to claim adv. / exempt status in	
	Math	English	Any subject	Math or English
	(A)	(B)	(C)	(D)
Panel A: Avg. among black SAT-takers				
B-W fr. minority in HS students' schools	3.9	-7.0	0.3	1.1
	(8.0)	(13.3)	(6.0)	(4.9)
B-W fr. minority in residents' census tracts	3.9	3.3	-8.4	-4.4
	(8.0)	(13.5)	(7.6)	(6.4)
Panel B: Avg. among white SAT-takers				
B-W fr. minority in HS students' schools	-18.2	-28.1	-11.1	-9.5
	(9.6)	(12.1)	(6.6)	(5.1)
B-W fr. minority in residents' census tracts	-12.9	-12.2	-17.7	-13.2
	(8.1)	(12.1)	(7.5)	(6.1)
Panel C: Difference between black and white averages				
B-W fr. minority in HS students' schools	17.5	13.6	17.9	15.1
	(8.5)	(7.7)	(5.7)	(5.3)
B-W fr. minority in residents' census tracts	10.8	7.7	10.1	5.2
	(7.8)	(9.4)	(6.5)	(5.4)

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. All columns include controls from column G of Table 4, though background measures in panels A and B are averaged over black and whites separately, rather than differenced as in Table 4 and panel C. All standard errors are clustered on the CMSA.

Table 10. Distinguishing effects of exposure to blacks from exposure to high-poverty neighborhoods on adjusted SAT scores

	(A)	(B)	(C)	(D)	(E)	(F)	(G)
Black-white difference: Fr. minority in	7.9	0.2	7.0	4.3	9.7	10.7	4.1
HS students' schools	(26.2)	(25.2)	(25.8)	(26.9)	(26.1)	(26.2)	(26.9)
Black-white difference: Fr. minority in	-92.0	-56.4	-94.8	-83.6	-76.6	-80.5	-67.7
residents' census tracts	(26.1)	(27.0)	(27.1)	(29.7)	(26.3)	(29.2)	(36.4)
B-W tracts: log(per capita income)		66.3					100.2
		(27.6)					(50.5)
B-W tracts: Male employment rate			-28.1				-30.6
			(63.6)				(64.2)
B-W tracts: Fr. of adults with less than				79.9			138.3
a high school education				(93.2)			(104.7)
B-W tracts: Fr. of adults with a BA or				118.8			-7.4
more education				(118.1)			(99.7)
B-W tracts: Child poverty rate					-102.5		7.8
					(77.0)		(150.3)
B-W tracts: Fr. of kids with two parents						44.7	11.6
						(49.0)	(93.2)
N	185	185	185	185	185	185	185
R-squared	0.76	0.77	0.76	0.77	0.77	0.76	0.78
p-value, school segregation effect=0	0.76	1.00	0.79	0.87	0.71	0.68	0.88
p-value, residential segregation effect=0	0.00	0.04	0.00	0.01	0.00	0.01	0.06
p-value, non-race tract exposure=0	0.00	0.02	0.66	0.60	0.19	0.36	0.06

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Control variables are those in column G of Table 4. All standard errors are clustered on the CMSA.

Appendix Table 1. Alternative estimates

	Base		Minority exposure							
	Base	CA/TX/FL indic.	Base	CA/TX/FL indic.	Elem. Schl. Seg.		College grads seg.		Separate B, W effects	
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)	(J)
School segregation measures										
Black-white difference: Fr. black in HS students' schools	-9.9	-5.2								
	(30.0)	(28.9)								
Black-white difference: Fr. Hispanic in HS students' schools	90.8	100.4								
	(59.5)	(60.0)								
Black-white difference: Fr. minority in HS students' schools			7.9	13.4	3.9		8.2	-11.0		
			(26.2)	(25.1)	(47.7)		(26.3)	(22.6)		
Black-white difference: Fr. minority in elementary students' schools					4.9	8.0				
					(52.2)	(29.1)				
Fr. minority in white HS students' schools									6.6	
									(77.2)	
Fr. minority in black HS students' schools									6.7	
									(27.9)	
Residential segregation measures										
Black-white difference: Fr. Black in residents' census tracts	-85.8	-88.3								
	(28.0)	(28.4)								
Black-white difference: Fr. Hispanic in residents' census tracts	-139.9	-149.1								
	(53.3)	(53.8)								
Black-white difference: Fr. minority in residents' census tracts			-92.0	-96.1	-93.1	-92.5	-72.3			
			(26.1)	(26.1)	(27.5)	(27.5)	(45.5)			
Black-white difference: Fr. minority in college degreed residents' census tracts							-25.1	-75.8		
							(45.8)	(27.2)		
Fr. minority in white residents' census tracts									45.2	56.2
									(91.5)	(22.1)
Fr. minority in black residents' census tracts									-97.7	-91.8
									(30.4)	(20.8)
CA/TX/FL		6.7		6.7						
		(5.3)		(5.4)						
N	185	185	185	185	185	185	185	185	185	185
R-squared	0.77	0.77	0.76	0.77	0.76	0.76	0.76	0.76	0.76	0.76
p-value, school segregation effect=0	0.30	0.25	0.76	0.59	0.93		0.75	0.63	0.97	
p-value, residential segregation effect=0	0.00	0.00	0.00	0.00	0.00	0.00	0.11		0.00	0.00
p-value, alternative segregation effect=0					0.93	0.78	0.58	0.01		

Notes: All models are weighted by $(N_w^{-1} + N_b^{-1})^{-1}$. Control variables are those in column F of Table 4. All standard errors are clustered on the CMSA.